# COMMENTARY ON THE SAILING RAFT, THE SWEET POTATO AND THE SOUTH AMERICAN CONNECTION

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PUBLICATION OF EXTENDED COMMENTARY seems desirable to restore a necessary balance to claims made in an article in the *Journal of Pacific History* on the "The Bamboo Raft as a Key to the Introduction of the Sweet Potato in Prehistoric Polynesia" (Langdon 2001). Among the issues raised, only the following five will be taken up here for critical discussion:

A) Competing hypotheses concerning the origins of the sail-rigged voyaging raft leading to its occurrence in South America – the new Langdon proposal, the older Doran view as recently reiterated by Green (1998:96-98) or even one of independent invention, perhaps with later influence through Polynesian contact.

B) A now very strong body of archaeological evidence attesting to a basic cultural continuity throughout the whole prehistoric Rapa Nui (Easter Island) sequence with the Pacific rat, the Asian jungle fowl and various Polynesian style artifacts present from its settlement period (800-1100AD). The source for this initial settlement was Mangareva. The contrast is with the Langdon (1997a, 1997b:71-72, 2001:68-70) account [drawing in part on Heyerdahl] for the first two periods – 800 to 1100 AD and 1100 to 1680 AD – where successive sets of culturally different settlers land on Rapa Nui from South America and live in isolation. Then Hispano-Polynesian intrusion occurs with total displacement that constitutes the basis for a quite different third period.

C) The central East Polynesian region (not Easter Island as the Langdon account would have it), as the locus for the initial introduction of the sweet potato [and the white-flowered bottle gourd] into the eastern part of the Pacific from South America.

D) The more usual proposals for linguistic sub-grouping within the Nuclear Polynesian languages against a failure of the 1983 Langdon and Tryon Futunic hypothesis to gain the same acceptance among linguists because of the much stronger data supporting the other choices. Also to be addressed is the important role played by Mangareva, though not Futuna, when identifying the Eastern Polynesian source for the Rapanui language.

E) The early presence of the bones of the red jungle fowl in archaeological deposits dating to between 1000 AD and 1200 AD in both Mangareva and Easter Island which can be traced to Polynesian, Lapita and ultimately Asian origins. The contrast here is with a postulated South American origin for the prehistoric chicken of Easter Island, combined with an argument that various Polynesian terms relating to chickens lend strong support to the Futunic linguistic hypothesis.

Let me examine each of these propositions in turn.

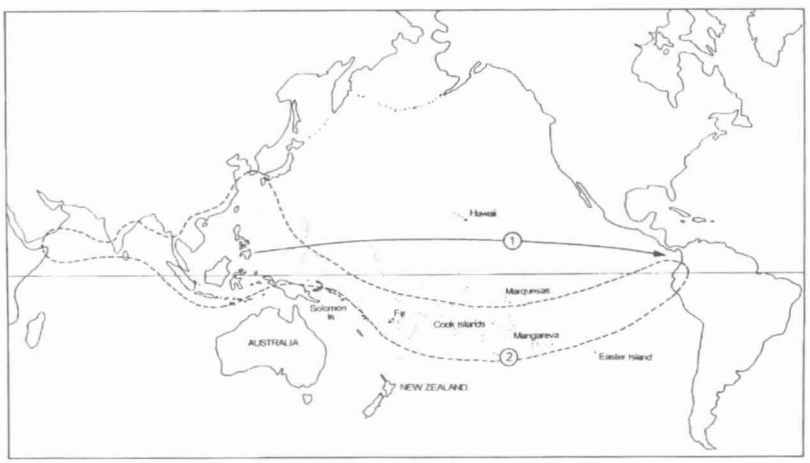


Figure 1. Ocean going sailing rafts: 1) a route proposed by Langdon (2001) for its transfer to South America in 200 BC; (2) its ethnographic distribution as mapped by Doran (1971: Figure 7.19).

## ASIAN ORIGINS OF THE SAILING CRAFT

A statement is made that "Western archaeologists have been apt to ignore its [the rafts] importance in Asian prehistory" (Langdon 2001:52). This certainly comes as a surprise to this archaeologist who has discussed this topic in recent literature on a number of occasions, as set out below. Two publications concern the bamboo raft's early role in the prehistory of Island Southeast Asia and Sahul, where the prior work of scholars such as Birdsell, Doran, Lewis and Horridge on the subject are drawn on and cited (Green 1991, 1994). Another, addressing the Austronesian antiquity and role of the sailing raft in the Taiwan-Philippine region (citing Blust 1999:74-75) and archaeological evidence for the early Neolithic exchange of goods in support of his views (citing Rolett et al. 2000:60), appeared in this journal (Green 2000a:74). A fourth forms part of an article by Green (1998) that is critiqued by Langdon in respect to other matters. Yet, he omits reference to the fact that this article also includes a distribution map for the sailing raft based on Doran, a drawing comparing the sailing rafts of Mangareva and South America at the time of European contact, as well as a textual discussion of this topic. Just like the Langdon account, an explicit concern was probable Polynesian contact with South America and the transport of both sweet potato and white-flowered bottle gourd from South America to eastern Polynesia. In sum, what we now have are two competing hypotheses for the Asian origin of the sailing raft in South America, as displayed in Figure 1, and its subsequent involvement in the transportation of the sweet potato to eastern Polynesia.

The Langdon proposal is for a nonstop voyage on a bamboo raft of 9500 miles from Mindanao in the Philippines direct to South America at 200 BC. As a rough guide, taking the cal-

culation provided in his article (at a speed estimated by Doran, and assuming conditions being comparable), a voyage of 9500 miles would have taken something like 136 days, since 4750 nautical miles from Taiwan to Seattle was estimated to take 68 days (Langdon 2001:56).

The Doran/Green option also assigns the origin of the raft to that Asian region with a late Pleistocene antiquity, though it is not specific as to where or just when this vessel form acquired a sail. However, the raft with the sail is currently seen as composing part of the Austronesian and Malayo-Polynesian vessel complex (Pawley and Pawley 1998:184; Blust 1999). Given the wide distribution of the sailing raft illustrated by Doran (1971), the idea and various physical versions of the sailing raft were spread by the Malayo-Polynesian descendents as they moved eastward into Near and then Remote Oceania as far as Eastern Polynesia (Figure 1). Examples of rafts in southeast Polynesia include the large log-platform vessels with triangular sails in Mangareva. While at the end of the Mangarevan sequence, rafts were seemingly only in use in its lagoon, oral tradition indicates they were also once used in ocean voyaging. They could easily have been used in the Mangarevan-Temoe and Pitcairn group open-sea interaction sphere that involved numerous well-documented exchanges of goods between 1000 AD and circa 1600 AD after which Pitcairn and Henderson were abandoned (Weisler 1996,1997). Van Tilburg (1998:144) also makes a sensible suggestion that palm log rafts may have once been present in Easter Island before the Chilean oil palm of that island went extinct, leaving only *nga'atu* (*totora*) reeds from which to manufacture a small float type raft-like vessel seen at contact.

A more provocative proposal was then advanced (Green 1998, 2000a:74) that from southeastern Polynesia the idea of an ocean-going sailing raft, if not an actual vessel, was taken by Polynesians to South America. There, balsa logs were substituted for wooden ones, in the same way as bamboo had already been replaced in Mangareva by wooden logs, bamboo being the earlier wide-spread Oceanic option favored for Pacific rafts. Having made a double canoe voyage of 1400 nautical miles to Easter Island from Mangareva (Finney 2001), its circa 800 AD Polynesian inhabitants only needed thereafter to make a further one of circa 2300 additional nautical miles to reach South America. Under simulated 1979 El Niño conditions, if "navigators from Easter Island or other eastern Polynesian islands had sailed east into the unknown by relying only on winds as the driving force they would have come close to the South American continent within four weeks" (Caviedes and Waylen 1993:132). Under more normal conditions, not more than a month from Easter Island or two months from Mangareva would suffice (Irwin 1992:164) to introduce the idea of the raft to South America, or to build one of their own forms of raft using balsa logs this time to return to eastern Polynesia.

This scenario avoids a 9500-mile direct voyage. It also avoids the need to assign the presence of the raft in South America to 200 BC. Support for that is largely by inference from the widespread movement of items of exchange, although numerous such items, (including those cited by Langdon), have for thousands of years moved very widely over land both within South American and to Central America (Bittmann 1984). Many examples of land-based contacts and interaction are known: corn, bottle-gourd, idea of pottery and trade-ware pots, metallurgy and metal objects, bark-cloth, minerals and semi-precious stones, high value trade shells, etc.

In addition, an inference that some kind of watercraft may have been available and involved in the transport of items along the South American coast, as well as in offshore marine exploitation, has long been a part of the history of this region's archaeology. Langdon refers to a few such sources, and a great number more could be cited. But, inferring from certain of these archaeological transfers just when large long-distance ocean-going sailing rafts (as opposed to other sea-going watercraft) first developed is rather more difficult to ascertain, and certainly too contentious to allow one to wholeheartedly adopt the 200 BC date nominated by Langdon.

Bittmann (1984), who published one of the more useful overviews of the topic known to me, describes and illustrates a simple five-piece model of wooden raft found at the site of Cañamo in Chile dating to the 8th century AD. This is the earliest secure archaeological manifestation known for what could have been a small predecessor to the large and complex balsa rafts seen during the early period of European contact in Northern Peru and Ecuador. From his review it would seem that the earliest indisputable indication for knowledge of watercraft, based on archaeology, occurs in Northern Chile during the first few centuries of the Christian era in the form of model boats. But he does not think actual watercraft based on these raft models would have been suitable for long distance voyages in the open sea. Still, from the indirect data arguments he adduced that coastal watercraft such as represented in these models were present even earlier in the region of northern Peru and Ecuador, along with northern Chile.

All this adds up to a third option requiring consideration. This is that a simple form of raft type watercraft, as in the models, evolved along the west coast of South America independent of those in Asia. Of course, this would not in itself rule out some later Polynesian influence in that process of further development of the ocean-going sailing rafts of Ecuador with their triangular sails rather like those known from Mangareva. The various forms of prehistoric pottery in the Galapagos more correctly derive typologically from the Sechura-Santa Elena region of the coastal South American mainland. They should probably be dated to between 1200 AD and 1400 AD rather than the earlier 10th century AD "Tiahuanaco" or Middle Horizon (Lanning 1970:177) as Langdon (2001:58) would have it. It certainly provides a fairly convincing minimum antiquity for the existence of full-fledged ocean-going sailing rafts. How many more centuries should be added is what remains open to debate.

In short, several solutions beyond that promoted in the article under review can be outlined which would account for the presence of the ocean-going sailing raft in South America. More importantly, due consideration of each is necessary to give balance to the discussion. When that is done, it appears to me that the suggestion for a very long distance direct voyage from the Philippines to the Guayaquil region of South America at 200 BC does not at present exhibit the same merit as other possibilities.

## A Polynesian continuity throughout Easter Island prehistory

Numbers of in-depth treatments of Easter Island prehistory have appeared in the last decade (e.g. Bahn and Flenley 1992; Lee 1992; Fischer 1993; Van Tilburg 1994; Martinsson-Wallin 1994). They make it superfluous to restate in any detail the now abundant archaeological evidence in support of a fundamental continuity to its cultural sequence. The island's settlement period of 800 to 1100 AD as defined by the Kon Tiki Museum archaeologists (Martinsson-Wallin and Wallin 1998) is based on the recent excavations at 'Anakena. Significantly, in terms of the initial inhabitants' origins, the assemblages from this period include the bones of the Pacific rat and the red Asian jungle fowl. They occur in association with a typical Polynesian one-piece circular bone fishhook and coral files to manufacture such items, polished adze flakes, a polished flat grinding stone, and obsidian flakes (Green 1998:103-107 and Figure 12). These items all continue on into the 1100 -1680 AD sites of the middle image *ahu* period, where typical Polynesian adzes and pounders in stone are also encountered, as are further chicken and rat bones. All carry on into the late period.

In this context of Polynesian continuity it is important to note that the circular one-piece fishhook in stone, a harpoon head in bone of a type identical to earlier ones found in the Marquesas (Sinoto 1996:142), as well as a bone example of an imitation "whale tooth" pendant have also been found in the circa 1200 AD assemblages excavated by the Kon Tiki Museum team (Martinsson-Wallin and Wallin 1994). Add to this repertoire the fact that the earliest settlement period religious structures of Easter Island, along with the various Proto Eastern Polynesian names for their component parts, fit comfortably into the early Eastern Polynesian *marae* complex (Green 2000b). Arguing, as Langdon has, that early inhabitants of Rapanui were strictly Indian populations from South American just no longer makes sense in light of so much contrary hard archaeological and linguistic data.

Heyerdahl, whose work Langdon cites (2001:68,70), is the authority for their jointly held view that American Indians were the island's sole inhabitants during the first two periods, with Polynesians only reaching those shores at the end of the second period. Langdon goes further to again advance a long-standing claim, unacceptable to many, that these Polynesian arrivals were of a very mixed ancestry – South American Indians mixed with western Polynesians in central eastern Polynesia – who then subsequently mixed with male European sailors (see Langdon 1996:195 and Footnote 2). Thus, when this group eventually reached Easter Island in the 17th century AD via Ra'ivavae in the Austral group, they carried certain distinctive but fairly widespread western European genes to Easter Island at that time. Yet, none of this finds direct support in the now intensively studied human skeletal collections from this period. Those of Rapanui are not only typically Eastern Polynesian, but are very like Mangarevans in their characteristics, and moreover, without any trace of a previously supposed South American element among them (Chapman 1998; Stefan 2000; Green 2000a:72-73).

The HLA genes Langdon mentions, largely known from one lineage of the present-day population of Easter Island, are probably the result of 18th or early 19th century European contact, although pinpointing such a source, as Langdon (1995a, 1995b) rightfully argues, remains problematic. What seems unlikely is that the HLA genes track back to a rather questionable and indirect 1680 input, when the population on Easter Island at that time and for many years thereafter, on all biological evidence, was fully Polynesian, both in a genetic sense (Hagelberg 1995) and osteologically.

In short, the hypothetical San Lesmes invasion and total replacement hypothesis in respect to Easter Island population circa 1680 has, for most, always been irrelevant to assessing the biological make-up of Easter Island's population of the earlier two periods. On the other hand, the strongly supported Polynesian continuity hypothesis from the time of first settlement makes the genetic and osteological evidence from the late period, beginning circa 1680 AD, highly relevant to also inferring the likely biological makeup of populations from those earlier periods. It is just fanciful to suggest that there were thousands of American Indians present on Easter Island who began to "die like flies" once their long isolation was broken in 1680 (Langdon 1995a:13). The dying was done by resident Polynesians after 18th century European contact, as the remains of their bones from that period attest. They were not, on the extensive data analyses that we have, of the kind of mixed genetic origins Langdon would have us believe.

In respect to continuity versus replacement, one should not overlook the changed views of the Kon Tiki archaeologist Skjølsvold who accompanied the Heyerdahl expedition to Easter Island. With his colleagues from the Kon Tiki Museum he has since conducted excavations at 'Anakena that have led to his publishing (1994:115) a different perception of these matters. That view may be summarized as follows:

The first settlers of Easter Island, originating in Eastern Polynesia and bringing with them the Pacific rat, did not build monumental forms of religious structures. Rather, the monumental forms appeared well after settlement – i.e. circa 1100-1200 AD. The first settlers and those responsible for the image ahu constructions "may have had different origins", although there is "no difficulty in seeing the similarity between the construction layout of east Polynesian *marae* and Easter Island *ahu*".

Like Martinsson-Wallin (1994), who canvasses in depth other possible South American parallels in addition to those incorporated in the image *ahu* at this time (1100-1200 AD), Langdon (1997:72) too nominates this particular interval in the Rapa Nui sequence as one when the island became home to a second band of American Indians with origins in the Tiahuanaco culture centered in the high Andes. Heyerdahl on the other hand, now that he and his team have carried out excavations in South America at the coastal Lambayeque River site of Túcume dating to 1100 AD and after, are more inclined to point to that region's culture as the likely source of this second band (Heyerdahl et al.,1995). A recent overview covering this point, perhaps too emphatically, appears in Johansen (2000). Whatever framework various scholars employ, this is an interval when many agree [except a few Polynesianists who still opt for total isolation after initial settlement] that the issue of influence on, or even settlement in, Easter Island from South America constitutes a justi-

fiable concern. Thus it remains an open issue worthy of close analyses and further discussion. For the Polynesian continuity advocates it can easily be ascribed to influence through contact with South America by Polynesians. For others it will require the provision of diagnostic physical, biological and linguistic data confirming that a band of American Indians was actually in residence.

### Central East Polynesia as the focus for the introduction of the sweet potato

Attempts such as Langdon's to introduce the sweet potato to eastern Polynesia solely through a sailing raft voyage from South America to Easter Island are not well-motivated (Yen 1974:308-311). The evidence of the degree of variation in multiple characteristics of this plant in the Pacific, extensively studied by Yen (1974), will not accommodate such a proposition and still account for its variation in Polynesia. Thus, Yen (1974: 260) writes:

> In separating out the varietal populations within Polynesia, it would be difficult to cite the actual landfall of the sweet potato from South America. There is no single island group whose varieties exhibit the complete variability of the Polynesian population which would enable its identification as the primary acceptor area which acted as a subsequent donor for the far reaches of the plant distribution.

Hence, the famous illustration (Yen 1974:Figure 78), in which the core region for the origin of the sweet potato in central East Polynesia is defined as an ellipse encompassing the Cooks, the Societies and the Marquesas Islands. From it, one trajectory leads off from the Cook/Austral portion of the ellipse to New Zealand, where the plant arrived at the time of its settlement circa 800 AD. Another leads off from the Marquesas to Hawai'i, the latter currently thought to have been settled circa 700 AD (Masse and Tuggle 1998; Tuggle 2001), or perhaps somewhat earlier. Not being available at that time in the Marquesas, the sweet potato probably did not reach Hawai'i until circa 1100 AD (Hommon 1976:258-269; Kaschko and Allen 1978). A third arrow in the Yen diagram goes from the Marquesas direct to Easter Island. As this is no longer perceived as a feasible route, it would now have to be redrawn to show the trajectory going via Mangareva (Green 2000:71-72). What this entails is an onward transfer to an already settled Easter Island in time to enhance its economic base and help support the burst in monumental ahu building and statue manufacture that prevailed after 1100 AD (Yen 1974:310-311).

It is in this context that the $10^{th}$ to $11^{th}$ century AD dating of the earliest archaeologically attested presence of the sweet potato in the southern Cook Islands (Hather and Kirch 1991) lends strong support to the ethnobotanically based interpretation of the plant variation data complied by Yen. Moreover, Langdon (2001:60) misleads readers in implying the pre-contact Mangaian and other Cook Islands words for sweet potato were "*kumara* or a cognate" in which normally expected sound shifts apply. The pre-contact Mangaian and other Cook Island term was *kuara* (Shibata 1999:108; Yen 1974:339). This form of the word *kumara,* in which the "m" has unexpectedly gone to zero, is a rather singular sound shift also known in one early contact record for Tahiti, and as the usual term in Hawai'i (POLLEX 1998). It is also important to note that the shift from "k" to glottal stop in Hawaiian would probably have taken place after the sweet potato and the word for the plant reached there. Thus, it may well be that the sweet potato and a singular form of the word for it reached Hawai'i through a Cook Island/Tahitian interaction chain rather than from the Marquesas. This is known to be an entirely plausible transfer in terms of Polynesian voyaging practice. It is also quite consistent with the recent analysis of the data provided by archaeology, the presence of two different mtDNA Pacific rat populations in Hawai'i, and the existence of borrowings from Tahitic languages in Hawaiian (Cachola-Abad 1993; Matisoo-Smith et al. 1998; Marck 2000:116-117).

Responding to Langdon's use of Polynesian linguistic materials, and the often unusual interpretations that derive from it, is always a difficult matter. Hence, for the most part, orthodox comparative linguists tend to avoid providing extended critical discussion of his writings. However, before working through the next two issues to be discussed, something more general needs to be said. A closely argued, though not well known, critique of one paper (Langdon 1989a) ends with this fairly cogent assessment:

The work reviewed here fails to set out the data as it exists in common sources. It fails to consider the full range of explanations for most data distributions. It fails to properly use common linguistic terminology. And it demonstrates a very idiosyncratic view of language in prehistory that is inconsistent with notions of linguistic, demographic and other cultural behavior grounded in observation rather than disconnected speculation (Marck 1996:62).

What follows attempts a productive exposure to other choices and data than Langdon has presented in the article under review or those other works on which he draws. Thus, the focus is on current views of subgrouping within the Polynesian language family and then on how the chicken made its way into Polynesia.

### Polynesian subgrouping in relation to the Rapanui language

It was not this writer alone who surveyed Eastern Polynesian linguistic views on subgrouping in detail and in the course of it rejected the 1983 Langdon and Tryon Futunic hypothesis [as Langdon (2001:74) might have readers believe]. Clark (1983) also formally reviewed that study in some detail and found it unconvincing, especially its dependence on the retention of the Proto Polynesian glottal stop and the six lexical items cited in support of the Futunic/Easter Island proposal. Nor have other linguists much involved in the historical study of Polynesian languages, such as Bruce Biggs, Victor Krupa, Andrew Pawley, Jeff Marck and Steven Fischer, ever adopted this schema in their analyses. In fact, Biggs (1978) and Krupa (1982) long ago set out the bases for changes in Polynesian phonology. These included the bases for the existence of the glottal stop, not only in Proto-Nuclear Polynesian (where several languages, including East Futunan, are witnesses), but also in its descendent daughter languages, including Proto Eastern Polynesian. This has recently been summarized in Marck (2000:24-25). In the article under review, Langdon's (2001:74-75) re-

counting his version of the significance of the Proto Polynesian glottal stop simply fails to undermine these analyses, much less offer a more plausible account acceptable to other linguists.

This does not mean that things have stood still, however, in adding further refinements to subgrouping hypotheses in Polynesian. The older hypothesis with which the 1983 Langdon and Tryon Futunic proposal had to contend was the Samoic Outlier one of Pawley (1967). Now Wilson (1985) and Marck (1999a, 1999b, 2000) have put forward a solid body of data in support of a Samoic, and then under it an Ellicean, and then an Ellicean Outlier subgrouping hypothesis. East Polynesian now subgroups under Ellicean. This replaces the earlier Samoic-plus-all-the-Outliers proposal of Pawley in which East Futuna and 'Uvea were also included under that subgroup. Under the new proposal, a subgroup of Futunic and Futunic Outlier languages splits off prior to the Samoic and Ellicean ones. In this subgroup East Futuna, East 'Uvea and Rennell-Bellona all preserve the Proto Polynesian glottal stop, whereas seven other Futunic and Futunic Outlier daughter language members lose it. The irregular retention of the Proto Polynesian glottal stop in the language of Rapanui is the major evidence for its retention in Samoic, in Proto Ellicean and in Proto Eastern Polynesian. It is entirely lost in all languages of the Ellicean Outlier subgrouping, as it usually is in almost all languages of the Central Eastern Polynesian subgroup. In Rapanui too it is lost in some phonological positions, while in Marquesan it is very largely lost, though a small residue of words still retain it (Marck 2000:24 and footnote 10, 35, 69-71).

In short, the loss of Proto Polynesian glottal stop has occurred independently a number of times and in all major subgroups of the Polynesian languages. This makes its retention less than compelling evidence for the Langdon and Tryon Futunic hypothesis or some of the assertions Langdon makes on the subject in the article under review. What does constitute strong phonological evidence for subgrouping are quite other sound shifts, known since 1978 and thereafter right up to the time of their most recent presentation by Marck (1999a). At no point is this readily available data countered when the Langdon and Tryon Futunic hypothesis is defended as a sound proposal. Moreover, should the authors do this, they will also have to counter the pronoun, morphological and lexical features summarized by Marck (1999a, 1999b, 2000) that provide an extensive set of more strongly motivated shared linguistic innovations for current subgrouping proposals in Polynesian than any forwarded so far under the Futunic hypothesis.

A strong case has been made in a number of recent articles, monographs and theses for the settlement of Easter Island from Mangareva. This was recently summarized in this journal (Green 2000) with appropriate references to voyaging, oral tradition, linguistic, archaeological, paleobiological and plant data bearing on that deduction. Additional voyaging information, underpinning the settlement of Rapanui from Mangareva, has since been outlined in further detail by Finney (2001). In the same vein, in the field of historical linguistics, Fischer (2001) [who had previously advocated in his writings a fairly close connection between Mangarevan and Rapanui languages] has now advanced a refinement of the long-standing subgrouping within Eastern Polynesia. Rather than seeing the Rapanui language as an isolate from all other eastern Polynesian languages, Fisher's proposal exploits a comment by Marck (1999b: 120, Footnote 6) that " the only substratum hypothesis I can offer for any part of Polynesia is that there may be a non-Central Eastern Polynesian substratum in Mangarevan speech". Using the many doublets occurring in Mangarevan, Fischer (2001) has now analyzed that aspect of its language and shown there is indeed an older substratum in Mangarevan that groups it together with Rapanui as one of the first branches to differentiate from the other languages of Proto Eastern Polynesian. Later contact with the Marquesas, also substantiated through archaeology, modified the language of Mangarevan substantially, with the result that for a long time it has been misclassified as a Marquesic language instead. One may end this section on the 1983 Langdon and Tryon Futunic hypothesis by saying current Polynesian subgrouping propositions now available in the literature provide very much stronger support for other alternatives than those Langdon continues to favor.

### THE DOMESTIC FOWL AND HOW IT GOT TO POLYNESIA IN PREHISTORY

Langdon (2001:74) denigrates Green for ignoring his study advocating multiple origins for the domestic fowl in Polynesia. The problem is that this study (Langdon 1989b) has not achieved any general acceptance among Polynesian scholars because they found the views expressed and the linguistic evidence presented in it seriously defective and for these reasons they have not used it. Thus, while Langdon believes there were "three distinct breeds of domestic fowls in Polynesia", each with separate origins and distinctive vocabularies relating to them, the rest of us believe that the data indicate there was only one breed in Polynesia, the red jungle fowl (*Gallus gallus*) from Asia. Consistent with this proposition there is but a single coherent set of inherited vocabulary relating to words associated with the fowl. Most of these are of Proto Oceanic, Proto Polynesian and Proto Nuclear Polynesian antiquity, as reference to any version of POLLEX over the last decade would demonstrate (Appendix 1). Furthermore, that vocabulary set proves to be entirely compatible with each of the older Samoic Outlier, as well as the more recent Futunic and its Outliers and the Samoic and the Ellicean subgrouping hypotheses. Finally, a Langdon (1989b:190) assertion that "of the few possible sources of Easter Island's poultry terms, East Futuna has the best credentials on linguistic grounds", is mistaken. The terms for poultry and other birds lend no special support at all toward substantiating the Langdon and Tryon suggestions about their Futunic hypothesis in the way Langdon avers (2001:74). Nor do the Rapanui words he lists as relating to chickens reveal a major Eastern Polynesian component without links to cognates in Western Polynesia. Among those he cites, one or two such lexemes do turn out to be innovations of Eastern Polynesian. Others in his tables, those for 'yolk', 'tail feathers' and 'the fourth backward toe of a fowl' for instance, are due to a failure to consider all the evidence now gathered and recorded in POLLEX.

Given the repeated references to the blue-egg chicken in the article under review it should be noted that chickens in Polynesia at contact and for years afterwards, including those from Easter Island, were not reported as laying eggs with blue shells.

In fact, Wilhelm (cited in Langdon 1989b), in his experiments with the Easter Island breed of chicken he believed to be most closely related to the South American Araucanian fowl which did lay blue eggs, was not able to back-breed Easter Island hens which could lay blue eggs (Langdon 1989b:173). The main linkage turns out to be a rather restricted distribution feature of frizzle feathers, a mutation frequently encountered in those two particular modern day breeding populations. While Langdon (1989b) writes of these kinds of chickens as if they constitute a separate species, *Gallus inauris*, they are not. Easter Island's chickens are and were *Gallus gallus* from the time of first settlement, and it is not yet known just when and how those of the 20th century acquired the frizzle feather mutation, or another commonly spread one of much wider distribution, on which Langdon relies.

I turn next to speculations on the presence of the chicken in South America before 1519 AD. The use of various closely corresponding chicken terms does not constitute the irrefutable proof that Langdon (1989b) adduces from them for the presence of fowls in Quechua speaking groups along the western South American coast from Ecuador south before the 16th century AD. Nor can it really be used, in the way Langdon does, to attest to a spread of the fowl and these terms beginning some 4000 to 4700 years ago. Moreover, to accept that evidence as being proof of a pre-16th century AD date for the chicken would require its bones to be found in fair abundance in the pre-Inka as well as in the pre-European contact Inka period refuse heaps, which they are not. For chickens to have been introduced into the Guayaquil/Elena coastal region of Ecuador more than 4000 years ago would require that chicken bones turn up in the midden sites of Valdivian age. They should then occur far more widely in many post-Valdivian South American sites thereafter. Images of fowls might even be expected to appear in some of the art on the ceramics or in the textiles or in pottery effigy vessels. Again, nothing among the above possibilities has ever been reported, and if it were, it would certainly have been a kind of instantly circulated news of obvious importance.

Finally, the conjecture that the idea of pottery [along with the Langdon proposal for the chicken not made by Meggers, Evans and Estrada] was introduced to South America via a boatload of immigrants from Japan more than 4000 years ago has been rather thoroughly undercut. This was accomplished through recent excavations of additional sites in the Guayaquil Santa Elena region. There, Valdivian I pottery has been found which lacks those types of decoration on the Valdivian II pottery on which Meggers, Evans and Estrada relied to enhance their case (Damp and Vargas S. 1995:159). Furthermore, the Valdivian sequence now goes through some eight phases, and Damp and Vargas S. (1995:166) believe it to be almost certainly of an indigenous origin. An independent invention of pottery by indigenes in the New World has also received much support through the occurrence of pottery assemblages elsewhere in South America that are much earlier than Valdivia (Clark and Gosser 1995), especially in the lower Amazon basin (Roosevelt 1995). In short, a proposal which brings the idea of pottery from Japan, along with blue-egg laying chickens, is simply no longer credible in the light of an ever increasing amount of archaeological information.

A consequence of the above is that *Gallus gallus* chicken bones identified from a settlement period site of Easter Island could not derive from South America. Chickens, like the Pacific rat, might come from Mangareva in light of all the other evidence. Yet Langdon (2001:74), citing Langdon (1989b:184-85) as the authority, avers that Mangareva did not have chickens prehistorically. How he explains away a carefully reported presence of *Gallus gallus* chicken bones in the 12th to 14th century Mangarevan archaeological sites (Steadman and Justice 1998) certainly needs clarification. This is particularly the case as the topic of the chicken in Mangareva prehistorically is also explicitly taken up as an item for discussion by Green (2000:74), Moreover, there it was linked to the early presence of the Pacific rat as well as the chicken in excavated Easter Island sites. In the context of all else from there, Mangareva seems a much more likely explanation than the Langdon one for the origin of, and the continuing presence of, the prehistoric chicken on Rapa Nui.

In the view of most archaeologists and historical linguists, the red jungle fowl from Asia was introduced, from locations in Remote Oceania with Western Lapita sites, to the Fijian, Rotuman and core region of Western Polynesia by its initial Eastern Lapita inhabitants (Kirch 1997:211). It came along with an appropriate vocabulary associated with fowls (Appendix 1). Certain terms, such as Proto Polynesian **moa* for fowls and Proto Nuclear Polynesian **qufa* for hen, were innovations of a subsequent period. Langdon errs by interpreting the presence of the word *'uha* for hens in Rotuman as an indicator of a supposedly separate breed of cock-fighting poultry that came direct to Rotuma from the southern Philippines or northern Sulawesi. Rather, this word is a later borrowing into Rotuman from a Nuclear Polynesian language. This latter source is also the probable loan origin of the Rotuman word *moa* for chicken. The Proto Polynesian chicken, *Gallus gallus*, is well attested archaeologically in the Ancestral Polynesian sites of the period (Kirch and Green 2001:129). It then spread widely throughout Polynesia, reaching the Marquesas, Mangareva and Easter Island early in their settlement, in each location associated with inherited vocabulary appropriate to this distribution. Certainly a few words, such as **mamari* for the egg of birds and **reke* for their combs, were innovated along the way. However, in Eastern Polynesia, these do not reflect loans from contacts with Easter Island, or attest to the presence of a different kind of chicken from South America as the Langdon would have readers believe. Rather, there were reasonable grounds for this author to avoid discussing Langdon's unorthodox speculations on the Polynesian chicken until forced to by an outcry against his failure to do so.

## CONCLUSION

Five major themes have been selected for examination from among those featured in a recent article concerned with the sail-rigged ocean voyaging raft. They include evidence for contacts between the Pacific islands and South America, the involvement of the sweet potato through them, explanations for the presence of the chicken in Easter Island and Polynesia, interpretations of Easter Island's prehistory, and subgrouping within the Polynesian language family. In each instance, the

options that are considered include those explored in the article under review along with those that oppose what appears there and still others (involving a number of authors) that were relevant though not mentioned. In my view, in the articles on which this commentary has focused, additional assertions appear that could also be questioned, although probably to little further purpose. Instead, based on commentary on five major issues, better-motivated choices than those forwarded within the article being reviewed are seen to exist in readily available literature. What I have done is to highlight the situation and, in some cases, to expand upon other possibilities. Throughout, an objective has been not just to provide counters to various statements in the Langdon paper, a number of which have proven to be factually mistaken. It has also been to sketch in just what ways these issues are being informatively addressed elsewhere within quite different historical frameworks than those advocated within that article. Hopefully, this will help to enhance decisions on how next to proceed when these topics are further discussed, as they surely will be.

## APPENDIX: Polynesian and Oceanic Austronesian words associated with the chicken and other birds

*Fowl and cock*

Proto late stage Oceanic **toqa*
Proto Polynesian **moa*

NOTE: Under **toqa* Clark (1994:81) also refers the reader to the Polynesian word of that form meaning courageous or warrior and, as Langdon (1989b:185) documents, Proto Polynesian reflexes of **toqa* used as a modifier after reflexes for **moa* in Eastern Futunan, Samoan, Rapanui, Rarotongan, Tahitian and Tuamotu means a cock (literally a warrior chicken). *Moa* also occurs in Fijian as the word for a brown quail, in Gilbertese as a borrowed word for chicken (Harrison 1994:345), and as a probably borrowed word for chicken in Rotuman.

*Female of animals, including hen*

Proto Nuclear Polynesian **qufa*

NOTE: Langdon (1989b:185) data would add Rapanui and Rotuman to the POLLEX list. It is a recognizable borrowing in Rotuman.

*Cackle, crow, cluck (of fowls, especially roosters)*

Proto Oceanic and Proto Polynesian **kokoo*

NOTE: This root is also found in compound forms for the chicken in some languages of central and western Island Melanesia (*kokoraka, kokorako, kokoroku, kakaruk, kakaleko*) where irregularities in sound correspondences and the onamatopoeic nature of these forms reduces confidence in their cognation (Pawley and Green 1984:130; Clark 1994:81), and therefore the postulation of a Proto Oceanic form for the chicken from them. *Kokoo* is present in Rapanui, but went unrecorded by Langdon.

*Cackle of a hen*

Proto-Polynesian **koto*

NOTE: Although it is no longer retained in Rapanui, its presence in other Polynesian languages indicates its former existence in a language ancestral to Rapanui.

*Chirp, cheep (as of small birds, baby chickens)*

Proto-Polynesian **kio*

NOTE: No longer retained in Rapanui, its presence in other Eastern Polynesian languages indicates former existence in a language ancestral to Rapanui. Because a reduplicated form of this lexeme meaning 'a small bird or chicken' occurs in Fijian, it may actually be of Proto Central Pacific status.

*The nest of birds (including that of the chicken); hollow place*

Proto late stage Oceanic **opa*
Proto Polynesian **ofanga*
Proto Central Eastern Polynesian **koofanga*

NOTE: A metethesized reflex of **ofanga* occurs in Rapanui.

*The egg of birds, and in Proto-Polynesian the yolk of such eggs; the word *mamari meaning egg is a Proto Eastern Polynesian innovation*

Proto Oceanic **tolu(r)*
Proto Polynesian **tooua*

NOTE: The reflex of **tooua* in Rapanui is mistakenly restricted to Eastern Polynesian languages by Langdon. In some Polynesian languages **tooua* and **mamari* can also mean the roe of marine creatures.

*Conceal or cover*

Proto Oceanic and Proto Polynesian **tataqo*

NOTE: Four Polynesian reflexes have a meaning of sitting on eggs. Reflexes of the lexeme do not occur in Eastern Polynesia.

*Barb, prong, sharp pointed object*

Proto Oceanic and Proto Polynesian **tara*

NOTE: meaning includes the spur of a chicken, especially when it is combined with *moa* as in Rapanui and Rarotonga.

*Heel of foot, butt end of object*

Proto Polynesian **reke*

NOTE: Includes a chicken's fourth backward toe in Rapanui. The reflex of this form in Rapanui is mistakenly restricted to Eastern Polynesian languages by Langdon.

*Scrape aside, smooth by scraping, clear*

Proto Polynesian **salu*
Proto Eastern Polynesian **selu*

NOTE: Marck (2000:95-96) documents a > e vowel change in Eastern Polynesian, and the presence of the word in Rapanui, where the scratching of earth as a hen does is included in its meaning. Not listed by Langdon.

*Comb of cock, tuft of hair*

Proto Central Pacific **sope*
Proto Eastern Polynesian **repe*

NOTE: A semantic change occurs in Eastern Polynesian where the **sope* term in Rapanui and other eastern Polynesian languages has the general meaning of buttocks or rear end, or in Tahitian and Tuamotuan 'the tail of a bird'. **Repe* meaning a fringe or flap or loose attachment such as a bird wattle or comb was a Proto Eastern Polynesian innovation replacing the **sope* meaning.

*Tail of animal or bird*
Proto Nuclear Polynesian **waelo*

NOTE: The reflex of this form in the Rapanui language is mistakenly restricted to Eastern Polynesian languages by Langdon.

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